Our great traveller and naturalist, Mr. Wallace, says, that "during twelve years spent amidst the grandest tropical vegetation, he has seen nothing comparable to the effect produced on our landscapes by gorse, broom, heather, wild hyacinths, hawthorn, and buttercups." Mr. Robinson's aim is to make our gardens as beautiful as our hedgerows and woods; and to this end he would not have his favourite plants placed together indiscriminately in a bed; but, as far as possible, he would imitate the natural *habitat* of each species, and for this he gives full instructions in each case. Half the volume is occupied by a list of hardy exotic plants suitable for naturalisation in our woods, semi-wild places, shrubberies, &c., with directions for their cultivation; and we hope it may assist in again bringing the public taste to the culture of flowers beautiful not only in themselves, but from the historic associations connected with many of them. A. W. B.

Die Kleinschmetterlinge der Umgegend Münchens und eines Theiles der bayerischen Alpen. Von August Hartmann. 8vo, pp. 96. (Munich, 1871: E. Lotzbeck.)

THIS is a catalogue of the Micro-Lepidoptera of the neighbourhood of Munich, and of a portion of the Bavarian Alps, with indications of the plants on which the larvæ of the different species have been found feeding and of the times and places at which the species have occurred. In his Introduction the author describes the method which he adopts for killing and preparing the delicate little moths which form the subject of his book, and from this the collector of Micro-Lepidoptera may gain some important hints. He also notices especially the curious moths belonging to the group of Psychidæ, and those Tineidæ which resemble them in habits; and he fully confirms the statements of Von Siebold as to the occurrence of parthenogenesis in *Solenobia triquetrella* and *lichenella.* W. S. D.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.]

Eozoön Canadense

IT is now about five years since a series of communications to the Geological Society of London by Logan, Dawson, Carpenter, and Sterry Hunt, announced the discovery of organic remains in the Laurentian rocks of Canada. They were decidedly interesting, not only as attempting to show that the belts of limestone interpolated in the great beds of Laurentian Gneiss were organic in their origin, but also from the *art* which the authors displayed in the mode of placing their views before the public. The realistic manner in which the fossil *Eozoön canadense* is drawn as it were from the *life*, coupled with the fixed belief in most men's minds that limestone is necessarily organic in its origin, predisposed many to accept the theory without much inquiry. The reputation of Dr. Carpenter as a physiologist was alone considered sufficient to settle the matter. These views did not, however, long remain unchallenged, for in the following year Professors King and Rowney, in a communication "On the so-called Eozoönal Rock," detailed the elaborate investigations by which they arrived at the conclusion that the presumed fossil was purely a mineral production. The replies that naturally followed were literally little more than reiterations of previous statements, excepting in the important admission from Dr. Carpenter, that the several features (that is chamber casts, canal system, and proper walls) could be separately paralleled elsewhere. He, however, took his stand upon the combination of the whole found in the Canadian specimens. To the fatal objection that all had been obtained from metamorphosed rocks Dr. Dawson replied by producing a specimen from Tudor, Ontario, which Sir W. E. Logan goes no further than to declare is from *comparatively* unaltered limestone, but which Dr. Dawson considers furnishes a conclusive answer to all arguments drawn from metamorphism. Since then I am not aware that any further evidence in favour of the organic hypothesis has been made public. On the other hand, Professors King and Rowney announced in a paper, read before the British Association at the Liverpool meeting, that they had discovered the features of the so-called organism in the Ophite of Strath in the Isle of Skye, an altered sedimentary deposit of the Liassic age, in which evidence of its mineral origin was conclusively proved. Here, at present, the matter rests; but in my opinion ample materials exist for forming a judgment, not by reliance on authority but by independent reasoning. With this object in view, and with your permission, I will proceed to detail a few of the facts of the case. Before doing so I would, however, call attention to the strange absence of any allusion to obvious objections which characterises the first series of papers, and to the persistent begging of the question involved in constantly speaking of the specimens as undoubted fossils. The adoption of this objectionable practice under authority of such eminent names is prejudicial to an impartial judgment, as it indirectly influences the mind. I am quite willing to admit that there existed sufficient reasons for suspecting them to be fossils, but I submit that it is not philosophical to state so distinctly before a thorough examination of all the objections. For evidence of this having been done I search these papers in vain. How then can any one, accustomed to scientific methods of investigation, help suspecting that under all this scientific and pictorial use of the imagination there exists or lurks a fallacy?

First, then, the specimen from Tudor has to be disposed of; nor will this be difficult, for it is altogether a lame affair. It is admittedly not from an unaltered rock, so it is difficult to see even how it bears on the question. The distinctive features are also obscure, and the chambers not of the usual form and proportion. To call this *Eozoön canadense*, and then bring it forward as closing the discussion, is an amusing piece of controversial skill. When it is more certainly co-ordinated with the original specimens, it will be time to discuss it. I simply ask, would it have been pronounced organic had it been the only variety discovered? I think not.

The broad fact then remains unshaken that in unaltered rocks no Eozoönal structures have yet been discovered. On the other hand, in metamorphosed rocks such structures are abundant, and even Dr. Gümbel, of the Bavarian Survey, a believer in Eozoön, has been much mystified by finding its features in impossible places. Not only do we find it in the Laurentians, but in rocks of a much later date, but curiously only in those that have undergone alteration. If it be an organism, then hydrothermal action, it seems, is necessary to its development, not as one would suspect during life, but ages after its entombment in sedimentary deposits.

The prevailing infilling material of the "chamber casts" representing "the sarcode body of the animal," is also admittedly serpentine or some analagous mineral—a mineral that forms the basis of no known fossil, consequently we are to assume that at each period of the animal's existence, either the conditions were different to those under which others were fossilised, or that the original infilling has since been replaced by serpentine; and this, be it said, must always have happened in those rocks afterwards selected for metamorphism.

Again, as minerals of this description are never found in unaltered rocks, we must be prepared to believe in the curious coincidence of the same rocks, and only these, having, at periods widely separated in point of time, been selected for the preservation of the organism and the deposit of these minerals. Either we must do this, or be prepared to show that metamorphism must necessarily change the infilling of the "chamber casts" to serpentine. Which supposition is the wildest?

Still further, we must believe that not only has Nature so miraculously preserved her pet animal, but that she has also imitated the fossil organism in the same minerals in an altered rock, in a manner to justify such acute observers as Professors King and Rowney in considering the imitation identical with the thing itself. For in the altered portion of the rock at Strath, before referred to, we have the features of the Eozoön, while the unaltered portion, which it gradually shades off into, teems with characteristic Liassic fossils. Have these fossils been obliterated in the altered portion, or have we the Eozoön again conterminous with the metamorphism?

And, in conclusion, we must further admit that all these conditions have been fulfilled over wide areas and at periods remotely separated with unerring regularity whenever the Eozoön has made its appearance. Is not this an improbability amounting to the impossible? For my part, this negative evidence far

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outweighs the "determination of its foraminiferal affinities by a point no larger than a pin's head," and I feel assured that whenever impartial geologists take the question up the fossil itself will become extinct. T. MELLARD READE

Blundellsands, Liverpool, Dec. 12

The Difficulties of Natural Selection

MR. WALLACE'S frank acknowledgment, for which I thank him, that he had in his two previous letters misunderstood my line of argument in what I consider one of the most important points at issue between us, absolves me from the task of again defending myself from charges of error and self-contradiction. As, moreover, Mr. Wallace has not accepted my challenge "to explain the nature of the intelligence which was operative in the creation of man, and which is a principle unknown in the rest of the organic world," it is impossible to pursue further this branch of the question. All naturalists will look forward with the most intense interest to Mr. Darwin's long-promised work on Natural Selection as applied to Man. There are, however, one or two subsidiary points raised in the discussion, to which I shall be glad of the opportunity of briefly referring. Mr. Stebbing, objecting to my attempted parallelism between mimicry and instinct, says that "it can hardly be said to be proved" that the extraordinary resemblances occasionally found in the vegetable kingdom are not protective or mimetic. I certainly think it can be. When we find an almost absolute identity between the foliage of a plant belonging to Africa and another growing in South America,* we are certainly justified in saying that one has not imitated the other, and that it gains no protection from the resemblance. Mr. Carvalho again makes merry over what he calls "my" argument, that imperfect imitation is, to all appearances, not beneficial in the cases published by Mr. Weir. The argument is not mine. I simply recount the observations made by practical entomologists, undertaken at the suggestion of Mr. Wallace himself. Mr. Carvalho's argument, which follows, is an instance of how, when a theory is once adopted, every conceivable fact may, by its too zealous advocates, be twisted to support it. Had these twig-like caterpillars been rejected by birds, it would have been considered a triumphant proof of the theory of Natural Selection; the fact that "they are eaten with great relish," we are told is equally "really in its favour"!

Westminster Hospital, Dec. 17 ALFRED W. BENNETT

Is Mimicry Advantageous?

THE discussion of mimicry among butterflies, in the recent numbers of this Journal, has brought to my mind some considerations which seem to have been overlooked by those who have treated the subject.

Of the fact of mimicry there can be no possible doubt, and in some instances it is even more striking than has been asserted. For instance, in North America, Messrs. Walsh and Riley have pointed out the resemblance between *Danais Archippus* and *Limenitis Misippus*; they might also have shown that in the extreme southern states where *L. Misippus* occurs, and *D. Archippus* is replaced by *D. Berenice*, the colour of the mimetic Limenitis deepens nearly or quite to the tint of the southern Danais.

But of how much actual benefit to the mimetic species is this so-called "protective" resemblance? It seems to occur where it can be of the least possible advantage to the species. The great sources of destruction here, as in all groups of animals, are in early life. How large a proportion of the eggs that are laid by butterflies ever finally produce imagines? Let those answer who have attempted to follow their history in their native haunts. My experience leads me to believe that, at the very least, nine-tenths—perhaps ninety-nine hundredths—never reach maturity. Hymenopterous and dipterous parasites beset them at every step; the eggs, although so small and often heavily ridged, cannot escape the ovipositors of the tiny Pteromali; while in attempting to breed caterpillars taken in the field, the chance is so greatly against the evolution of a butterfly, that hymenopterists actually choose this method of supplying *their* cabinets. "Of two hundred larvæ of *Pieris Brassicæ*," Mr. Drewsen, of Denmark, writes to me, "I obtained only twenty pupæ; all the rest were attacked by *Microgaster glomeratus*," and my own attempts with the larvæ of *Pyrameis Atalanta*, both in America and Europe, have been even more unavailing. These caterpillars seem to be peripatetic banquetting halls of Microgasters and Tachinæ.

Now it is a curious fact that while the globular egg of *Limenitis Misippus*, with its deeply-pitted shell, defended by long filamentous spines, is constantly attacked by parasites; and the grotesque, hump-backed, strangely-coloured caterpillar of the same species is likewise infested to an extraordinary degree, I have been unable to discover by very careful search any evidence that the egg or larva of *Danais Archippus* is ever pierced by a parasite; yet the egg is not small and only lightly ribbed, and the caterpillar large, fleshy, smooth-skinned, and gaily banded, living on the widely-separated leaves of Asclepias, with no attempt at concealment. The abundance of the imago of the Danais is then due quite as much to the immunity of the egg and larva from the attacks of parasites, as to any freedom it may itself enjoy from pursuit by insectivorous birds.

Although I have hunted butterflies for fifteen years, I confess I have never seen one in a bird's bill, and my faith in that method of lessening their numbers is very slight. Birds, too, must be their greater foes in earlier life; and the chances of living, which are certainly against them before they take wing, seem afterwards rather in their favour, at least, until they have accomplished their mission.

If, then, such an extraordinary element as Mimicry is to be summoned to the aid of Natural Selection, and can perform its task in such a masterly manner, why has it been made to waste its energies upon unimportant material? If the object of the resemblance be protection, why does not the unfortunate caterpillar of the Limenitis mimic the more favoured larva of the Danais?

I cannot now consult the writings of Messrs. Wallace and Bates, nor do I remember their statements respecting the abundance of the mimetic species compared to that of its normal congeners. In my own country *Limenitis Misippus* is, as a general rule, more common than *L. Ursula*, but the difference in their numbers is not very marked. It is by no means as great as one would expect had Mimicry in the imago state so strong a protective power as has been assumed. Two closely allied species,* occupying the same geographical area, do not often occur in the same abundance, whatever be the cause; and the disparity in numbers in these two species of Limenitis is no greater than occurs in many instances where mimicry plays no part.

Cairo, Egypt, Nov. 9 SAMUEL H. SCUDDER

Nepenthes

THE allusion to Nepenthes in Mr. Buckton's interesting article in a late number of NATURE, on the liquid secreted by this and other plants, prompts me to place on record a few facts regarding that genus, at which I have just arrived, after monographing the Pitcher-plants for the "Prodromus Systematis Vegetabilium" of De Candolle; a work of which the publication is suspended, owing to the siege of Paris.

The genus *Nepenthes* extends from Madagascar on the west to N.E. Australia, the Louisiade Archipelago, and New Caledonia on the east; embracing within these limits, thirty species, most of which have well marked characters in the pitcher, but which, with only two exceptions, present a wonderful uniformity in the structure of both flower and fruit. It has two foci of maximum development; the Malay Peninsula (including Sumatra), and Borneo, in both of which localities the species are not only more numerous, but more gigantic than in any other country. No fewer than twenty-one species inhabit these two countries, of which thirteen are common to both; but, what is very remarkable, the intervening island of Java contains but one representative of the genus, and that a totally different species from either the Bornean or the Malayan; thus confirming the fact first brought to light by the Dutch naturalists, of the close biological relationship between the two former localities, to the exclusion of Java. Only one species has a wide range, the *N. phyllamphora*, which extends from Sumatra to Borneo, Amboyna, China, &c., but is absent from the island of Java.

Proceeding from the Malayan islands westwards, we find one species in east Bengal, more allied to the Javanese than to any other; another in Ceylon, the old *N. destillatoria* of Linnæus (a name long usurped in our gardens by the Bengal plant), which presents the first departure from the typical structure of the genus, having a spreading paniculate inflorescence; a character shared by those in Madagascar and the Seychelles. Proceeding further west to the African islands, we find still further deviations from the type, which now extend to the structure of the seed and

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* See NATURE, Vol. ii. p. 70.

* *L. Misippus* and *L. Ursula* can with difficulty be separated in their earlier stages, although so unlike in their perfect forms.

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